LETTERS TO THE EDITOR.

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The Sand-Blast.

AMONG the many remarkable anticipations contained in T. Young's lectures on natural philosophy (1807) is that in which he explains the effect of what is now commonly known as the sand-blast. On p. 144 he writes:—"There is, however, a limit beyond which the velocity of a body striking another cannot be increased without overcoming its resilience, and breaking it, however small the bulk of the first body may be, and this limit depends on the inertia of the parts of the second body, which must not be disregarded when they are impelled with a considerable velocity. For it is demonstrable that there is a certain velocity, dependent on the nature of a substance, with which the effect of any impulse or pressure is transmitted through it; a certain portion of time, which is shorter accordingly as the body is more elastic, being required for the propagation of the force through any part of it; and if the actual velocity of any impulse be in a greater proportion to this velocity than the extension or compression, of which the substance is capable, is to its whole length, it is obvious that a separation must be produced, since no parts can be extended or compressed which are not yet affected by the impulse, and the length of the portion affected at any instant is not sufficient to allow the required extension or compression. Thus if the velocity with which an impression is transmitted by a certain kind of sound be 15,000 ft. in a second, and it be susceptible of compression to the extent of 1/200 of its length, the greatest velocity that it can resist will be 75 ft. in a second, which is equal to that of a body falling from a height of about 90 ft."

Doubtless this passage was unknown to O. Reynolds when, with customary penetration, in his paper on the sand-blast (*Phil. Mag.*, vol. xlvi., p. 337, 1873) he emphasises that "the intensity of the pressure between bodies on first impact is independent of the size of the bodies."

After his manner, Young was over-concise, and it is not clear precisely what circumstances he had in contemplation. Probably it was the longitudinal impact of bars, and at any rate this affords a convenient example. We may begin by supposing the bars to be of the same length, material, and section, and before impact to be moving with equal and opposite velocities v. At impact the impinging faces are reduced to rest, and remain at rest so long as the bars are in contact at all. This condition of rest is propagated in each bar as a wave moving with a velocity a, characteristic of the material. In such a progressive wave there is a general relation between the particle-velocity (estimated relatively to the parts outside the wave) and the compression (e), viz., that the velocity is equal to ae. In the present case the relative particle-velocity is v, so that $v=ae$. The limit of the strength of the material is reached when e has a certain value, and from this the greatest value of v (*half* the original relative velocity) which the bars can bear is immediately inferred.

But the importance of the conclusion depends upon an extension now to be considered. It will be seen that the length of the bars does not enter into the question. Neither does the equality of the lengths. However short one of them may be, we may contemplate an interval after first impact so short that the wave will not have reached the further end, and then the argument remains unaffected. However short one of the impinging bars, the above calculated relative velocity is the highest which the material can bear without undergoing disruption.

As more closely related to practice, the case of two spheres of radii r, r', impinging directly with relative velocity v, is worthy of consideration. According to ordinary elastic theory the only remaining data of the problem are the densities ρ, ρ', and the elasticities. The latter may be taken to be the Young's moduli q, q', and the Poisson's ratios, σ, σ', of which the two last are purely numerical. The same may be said of the ratios q'/q, ρ'/ρ, and r'/r. So far as dimensional quantities are concerned, any maximum strain e may be regarded as a function of r, v, q, and ρ. The two last can occur only in the combination q/ρ, since strain is of no dimensions. Moreover, $q/\rho=a^2$, where a is a velocity. Regarding e as a function of r, v, and a, we see that v and a can occur only as the ratio v/a, and that r cannot appear at all. The maximum strain then is independent of the linear scale; and if the rupture depends only on the maximum strain, it is as likely to occur with small spheres as with large ones The most interesting case occurs when one sphere is very large relatively to the other, as when a grain of sand impinges upon a glass surface. If the velocity of impact be given, the glass is as likely to be broken by a small grain as by a much larger one. It may be remarked that this conclusion would be upset if rupture depends upon the *duration* of a strain as well as upon its *magnitude*.

The general argument from dynamical similarity that the maximum strain during impact is independent of linear scale, is, of course, not limited to the case of spheres, which has been chosen merely for convenience of statement.

RAYLEIGH.

The Earth's Contraction.

THE conclusion of the Rev. Osmond Fisher (NATURE, February 26) that if the moon originated as a detached portion of the earth, the earth's radius at the time (even allowing for the much more rapid rate of rotation indicated by Sir G. H. Darwin's researches) must have been about three times its present one, leads to a very interesting speculation, namely, as to whether the earth's radius may not have contracted very considerably *within the time represented by the known geological formations*. There is, I think, observational evidence which warrants us in believing this to have been the case.

Prof. Heim estimated the linear compression required to produce the Alps at seventy-four miles, which means a reduction of the earth's radius by twelve miles, or 0·3 per cent. Taking the whole of the existing mountain ranges, we may roughly estimate a total reduction of ten times this amount, or 3 per cent., as being indicated since the middle of the Tertiary epoch. Yielding of the earth's crust by intense folding has probably always taken place in particular areas, but it is a fact that as a whole the rocks show more and more folding, faulting, and overthrusting the farther back we go into the geological record, and the mountains formed in the older epochs have long since been removed by denudation, which was naturally most active where the plication was most intense. Taking the rock-structures alone into consideration, would any geologist who has worked extensively amongst the oldest fossiliferous rocks affirm that the evidences are against a contraction of the earth's radius of the order of 20 per cent. since they were deposited? A contraction of this magnitude would be accommodated by a continuous folding of the crust into anticlines and synclines at angles of 37° with the horizontal, and most accounts

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of the tectonics of the earlier Palæozoic rocks describe folding and shearing which approach this average, though, of course, there are some extensive areas where the disturbance has been much less. When we come to the Archæan rocks the case is even stronger.

A very simple calculation shows that the amount of contraction suggested above implies only a rate of shrinkage so slow that if going on at present it would fail to be detected by the most refined geodetic measurements repeated after a century. A contraction of the earth's radius of only three inches a year, which would cause a 20 per cent. diminution of the radius in about twenty million years (and this is probably a low estimate for the age of the Cambrian strata) would only change the absolute length of geodetic lines by less than one part in a million in a hundred years. Observations of the lunar parallax, which afford another theoretical means of detecting changes of the earth's radius, would have to be repeated at intervals of twenty-five thousand years in order to detect a single second of diminution, even if we could assume the moon's mean distance to be constant throughout this period.

If the hypothesis of a continuous contraction of the earth's radius at an average rate of somewhere about three inches a year throughout geological time is entertained, it not only furnishes a plausible explanation of the prevalent folding of the ancient rocks, but also tempts one to indulge in a number of other speculations, such as, for instance, how far the flying-powers of the great Mesozoic reptiles may have been influenced by the lesser value of gravity at the earth's surface with a larger terrestrial radius and a greater speed of rotation. But collateral issues may well rest until the main point has been argued, and as to this I should like the opinion of geologists whose experience of the older formations has been greater than my own. I am at present surveying in a disturbed country where I should certainly have had to correct my base-line very considerably for dislevelment if I had measured it along the planes of stratification of the rocks, and this circumstance has suggested the question to my mind on reading Mr. Fisher's letter.

John Ball.

Wadi Shellal, Sinai, March 31.

Zoological Classification.

The complaint, thus entitled, of Mr. H. C. Williamson in Nature for April 9, p. 135, is a fairly common one just now, and his way of putting it suggests some remarks from the other side.

The zoological classification of this or any other day is "satisfactory" in proportion, first, as the principles of that classification commend themselves to the general intelligence of zoologists; secondly, as the classification is in agreement with those principles. The multiplication of the units to be classified, whether subspecies, species, or genera, can have no bearing on the validity of the classification.

The object of classification is the arrangement, but not the nomenclature, of the units.

The older genera have been subdivided, not merely because the number of species has proved unwieldy (indeed, if the necessary bases be absent, such genera cannot be split, however much it be desired), nor merely because it has proved possible to group the species (for in this case subgeneric divisions are adequate), but because study has shown that the species formerly included in a genus have not really the close interrelationship which such inclusion implies, but are of very diverse descent. In nomenclature as affected by classification most of the serious difficulties which happen to have come under my own notice are due to our increased, but still imperfect, knowledge of origins.

The real trouble and source of disagreement between zoologists is that some (and especially the "applied" workers) look at taxonomy with the eyes of the old school, which regarded only the amount of resemblance in structure, while others follow the new school which seeks to express history and relationship. In a word, there are two sets of principles, and our classification is, if not halting between the two, at least passing rather lamely from one to the other. And as for nomenclature, the trouble is that the Linnean system was devised for a classification on the old principles, and one often doubts how far it is applicable to the new method.

When the nature of our difficulties is clearly understood, nobody will propose to effect progress by such retracing of our steps as "the extinction of half at least of the genera." In ordinary writing one practical way of getting round the difficulty is half hinted at in Mr. Williamson's last sentence; it is to use the family name. Thus we can speak of a Cidarid, a Terebratulid, a Phacopid, a Pentacrinid. Or the transition may be eased by using the old name with some such *caveat* as *sensu lato, e.g.* Rhynchonella (s. l.), Antedon (s. l.). In writing the "Guide to the Fossil Invertebrate Animals in the British Museum" the conflict between accuracy and intelligibility was settled by adding the older better-known name within square brackets after the correct or modern generic name, *e.g. Dalmanites* [*Phacops*] *caudatus.*

F. A. Bather.

Natural History Museum, April 11.

Electric Emissivity at High Temperatures.

We have recently conducted at the National Physical Laboratory some experiments on the emission of electricity from a number of substances at very high temperatures—between 2000° C. and 2500° C. The experiments were carried out in a carbon-tube resistance furnace at atmospheric pressure. Among the substances tried were the alkaline earths and a number of metals. In every instance the temperature was sufficient to vaporise rapidly the substance under test, and, under these conditions, very large amounts of electricity were emitted. For example, barium oxide emitted negative currents of the order of 4 amperes per sq. cm., while boiling tin gave currents of about 2 amperes per sq. cm. No external potential was applied in any of the experiments.

We hope to publish very shortly a full discussion of the results.

G. W. C. Kaye.
W. F. Higgins.

National Physical Laboratory, Teddington,
Middlesex, April 22.

An Optical Illusion.

Some days ago I was reading, the sunrays falling aslant on my forehead, it being about five o'clock p.m. After having read awhile, the letters showed a vivid red, the paper itself retaining its white colour. The rays did not fall on either of the two sides of the page. When I used my hand to shade the eyes, the letters *immediately* became black again. On removing my hand, it took them some seconds to change from black through dark red to bright red. I may add that I am short-sighted, but was reading without spectacles.

I should feel obliged if some reader of Nature would give me an explanation of this.

J. W. Giltay.

Delft, Holland, April 11.

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